said to have been observed in birds, viz., that when a hen is fertilised by a cock of another kind the resulting egg is contained in a shell tinted, more or less, like those laid by the cock's own breed. At the time, I must confess, I was rather inclined to doubt if it did really occur, or if it were not a simple reversion, or a mistake, when my attention was drawn still closer to the subject by a friend who had kindly offered to assist in obtaining, if it were possible, additional proofs of telegony by first crossing a canary hen with a greenfinch cock and then returning her to her own breed. This was done, and resulted in three eggs being laid to the greenfinch. Their shells were all tinted more like the eggshells of a greenfinch than those of a canary. Two of these eggs were afterwards found to be infertile. This showed that the alteration in the tint of the eggshell had nothing to do with the nature of the fertilising spermatozoon. But the occurrence of hybrid oochromy could not be said to have been proved, for there is very little difference in the tinting of the eggshells of a canary and greenfinch, and I do not know whether the canary was purely bred or not.

I was thus anxious to find out for certain whether or not such an occurrence was possible. I therefore obtained three black Minorca hens, which had come of stock that had been purely bred for the last twenty years. The Minorca breed is the oldest variety of the famous Spanish fowls, of which the origin seems older than the recollection of it ![1] These three Minorca hens I penned up alone for more than four weeks, during which time thirty-two eggs were laid, and the shells of all of the later ones were of a very pure white colour.

The reason I had kept them alone for so long a time was that I required eggs entirely free from the intervention of any cock, and the commonly accepted opinion of poultry fanciers seemed to be that a period of nearly three weeks was necessary for the complete extermination of spermatozoa. However, to prevent any mistake, at the end of this time three eggs were artificially incubated for a period of forty-eight hours at the Durham College of Science, and they proved quite infertile.

After having thus demonstrated that the Minorca egg is contained in a pure white shell, I introduced into their pen a buff cock of the Cochin China breed, a breed famous for the brown with which its eggshells are tinted. The second egg laid after its arrival in the pen was provided with a shell of a very decided brown tint, and among a dozen or more laid within the succeeding two or three weeks, the shells of several were of a faint brown tint.[2] I was, however, unable to observe any difference in the microscopic structure of the eggs, such as is described by Herr von Nathusius. (See "Dictionary of Birds," by A. Newton, p. 190.)

This remarkable case appears to me to be an almost incontestable proof that hybrid oochromy does, at times, occur, as the only other way for accounting for pure bred black Minorca hens laying brown tinted eggs would be that they were reverting to some brown-egg-laying ancestors, a very unlikely supposition when we remember the age of the breed.

The next question to answer is—How does hybrid oochromy take place? I feel quite convinced, both from my own observations and those with the above-mentioned canary, that the tint of the eggshell is not, and cannot be, affected by the nature of the fertilising spermatozoon, and so we must turn our attention to the spermatic fluid, the chemical properties of which, acting in conjunction with those of the products of the shell-gland, will probably be found to be sufficient to cause this change of tint.

Hybrid oochromy has, in company with a closely associated phenomenon in another kingdom (I refer to Xenia), often been referred to as a case that cannot be explained by the Weismannian theory of heredity, *i.e.* the continuity of the germ-plasm. If the above explanation (and I can suggest no other) of hybrid oochromy should be proved to be correct, it is easily seen to be merely a chemical change and wholly apart from the phenomena of fertilisation. In the same way I should think it is very possible that xenia might be found to be not unconnected with the conjunction of the male and female elements forming the endosperm. It doubtless will be shown before long whether or not these two attempted explanations be correct. They will, I hope, however, tend to lessen the opposition to the Weismannian theory by showing how a fact which, at first sight, appears absolutely antagonistic thereto is found to be in complete accordance with it. It also shows what a deep effect may be induced in living organisms by the interaction of the chemical products of their glands.

I must here take the opportunity of expressing my best thanks to the Durham College of Science, Newcastle-on-Tyne, for allowing me the ground, &c., on which to conduct the experiment. G. P. Bulman.

Newcastle-on-Tyne.

[1] "The Poultry Book," by Lewis Wright. Popular edition, p. 340.

[2] Since writing the above I have incubated two of these eggs and found them fertile. At first sight this would seem to contradict the explanation given, but although I hold that fertilisation is not necessary, it certainly may take place in some cases.

The Swimming Instinct.

I have just tested the inherited powers of swimming in newly hatched pheasants. I find that when placed in tepid water, at the age of about thirty hours, they swim easily with well-co-ordinated leg-movements and show very little signs of distress.

C. Lloyd Morgan.

University College, Bristol, June 24.

RECENT SCIENTIFIC WORK IN HOLLAND.

Beginning with that which is of most general importance, we draw attention to the recent work of Prof. Hugo de Vries, of Amsterdam. Prof. de Vries, who is well known as a botanist and biologist and whose name is familiar to those acquainted with the history of modern chemistry, has just published the first part of a book entitled "Die Mutationstheorie. Erster Band. Versuche und Beobachtungen über die Entstehung von Arten im Pflanzenreich" (Leipzig: Veit, 1901), containing, as the title indicates, the account of a series of observations on the formation of new species in plants. Starting from the fact, well known to florists, of the appearance of "single variations" in their flower-beds, de Vries has been trying to find wild flowers which would show the same phenomenon. Of the 100 species investigated only one appeared to possess the property which was looked for, the *Œnothera Lamarckiana*, originally from America, but at present growing wild in Holland. Now about ten years ago de Vries transferred specimens of this plant to the botanical gardens at Amsterdam, and up to date he has studied as many as 50,000 of its descendants.

Of these 50,000 about 49,200 were in no respect different from the original patriarchal *O. Lamarckiana*, showing no tendency towards gradual change in any special direction, but only the common small fluctuating "variations" as regards size and appearance on either side of a normal, in fact resembling in that respect other plants and animals which are left to themselves without being interfered with.

Quite otherwise the 800 other plants. None of these, although appearing spontaneously, could be said to be representatives of the species *Lamarckiana*, from which they were descended. De Vries arranges them in seven distinct species, viz. 1 of *O. gigas*, 56 of *O. albida*, 350 of *O. oblonga*, 32 of *O. rubrinervis*, 158 of *O. nanella*, 221 of *O. lata* and 8 of *O. scintillans*. Now comes the crucial question of the whole investigation. What right has de Vries to look upon the differences between these seven species and the original species as being of a different order from the variations between the specimens of each species, and what entitles him to call these differences *mutations* as opposed to variations? The answer is this: a representative of these new species produces descendants the majority of which unmistakably belong to the same species as itself. Not all the new species behave in the same way; as an instance, the only representative of *O. gigas* was isolated and made to fertilise itself. From it were obtained 450 plants, all of which, with only one exception, were *O. gigas*, the one exception not being a return to *Lamarckiana* but belonging to a new variety. The plant is a strong one and retains its properties in subsequent generations so far as investigated.

The *O. albida*, on the other hand, which appeared frequently, is a weak plant, not very fertile, but perfectly constant so far as it went.

The last species in the above list, the *Œnothera scintillans*, differs from the others in this respect, that it is extremely unstable, *i.e.* possesses the property of mutation to a high degree, a large proportion of its descendants belonging to other species, specially *O. oblonga* and *Lamarckiana* itself.

Want of space prevents us from going into further details. Enough has been said to show that de Vries has evidently made a momentous discovery. So far as his observations go, new species appear *suddenly* by *mutation*, never as the outcome of a progressive *variation*. With legitimate pride the author declares that he has been able for the first time to watch the formation and development of new species. The facts are so striking and convincing that an outsider like the reviewer cannot but feel that a new period in the theories of the origin of species and of evolution has been inaugurated.

As we saw, some of the new species which made their appearance did not seem to be inferior in stability to the mother-species; on the other hand, one of the species, the *O. lata*, only appeared as female plants without pollen, and the *O. albida* did not show the same vitality as the others and was evidently doomed to disappear again. The observations, therefore, do not support the idea that in the formation of new species Nature is carrying out a definite plan; on the contrary, it all looks like accident. A new species may be one strong and fertile enough to remain, and possibly, under favourable conditions, replace the mother-species, but it may just as well be a sickly kind without any chance in the struggle for existence. For the struggle for life between individuals de Vries substitutes the struggle for continued existence between species, the new species always appearing suddenly.

De Vries' views are thus directly opposed to the common form of the theory of evolution; not that the importance of the single variations had escaped attention altogether, but they were always lost sight of, and prime importance is generally attached to the selection through the ordinary variations. De Vries' experiments support the results arrived at by Scott and other palæontologists that there is no evidence in the successive strata of the earth of a gradual development of one species into another and that everything points at small but sudden transitions.

It can hardly be believed that the species which de Vries happened to come across can be the only living one possessing the property of mutation, and men of science may therefore look forward to a new period of extensive research on the lines of de Vries' work. One feels that new life has been infused into the problem, and that tangible facts are now available and experiments which will replace a good deal of rather empty theorising and hollow controversy between rival speculations.

Turning to physical research we naturally think, first of all, of the discovery made by Prof. Zeeman some years ago when still assistant at Leyden in Prof. Onnes' laboratory. This discovery of the influence of a magnetic field on the period and character of light radiated by a source in the field came just at the right time to bring the theory of ions or electrons into prominence, a theory the necessity of which had already appeared in many ways, and which had been worked out for the first time by H. A. Lorentz. In fact, without any calculation it is easily seen what influence a magnetic field must have on the light-vibrations, if these consist in the vibrations of charged particles. We have only to resolve the vibration of the electron in the direction of the field and at right-angles to it, and, again, the latter component into two circular vibrations of opposite directions, to see at once that the magnetic force must increase the centripetal acceleration in the one and diminish it in the other circle without affecting the third vibration; thus the ordinary doublet in the direction of the field, and the triplet in a direction at right-angles to it, both with their proper states of polarisation, may be directly inferred from the theory in its simplest form. The direction of the circular polarisation of the doublet shows the preponderance of the negative over the positive electrons in producing the phenomenon, and from the magnitude of the change in wave-length, *i.e.* the width of the doublet, the ratio of the charge of the electrons to their mass can be inferred.

It need not be here explained how these conclusions were confirmed by, and confirmed results obtained in the study of the conduction of electricity in gases, by J. J. Thomson and his pupils and others. Zeeman's phenomenon soon became public property, and has since been developed by many others as well as by Zeeman himself.

H. A. Lorentz, whose name is connected with Zeeman's discovery and its immediate explanation, published the first complete account of his electron-theory in 1892 in French, and a more complete version in 1895 in German ("Versuch einer Theorie der electrischen und optischen Erscheinungen in bewegten Körpern." Leyden: Brill). To this theory Lorentz was led by his discussion of aberration phenomena; there is, perhaps, no phenomenon which is so readily explained in elementary text-books but gives so much trouble when properly discussed as aberration.

Lorentz's researches led him to adopt Fresnel's theory, which assumes that the ether is at rest and that bodies move through it without disturbing it. This theory is in accordance with the negative results of Lodge's well-known attempts to put the ether in motion by spinning two heavy wheels close together in the same direction. Starting from this hypothesis and assuming that all electric phenomena—including light—in bodies are due to the presence, motion and vibration of electrons acting on each other through the ether, Lorentz developed a theory which leads to the proper Maxwell-equations for bodies at rest, and, moreover, explains the great majority of the experiments and phenomena relating to moving bodies—such as aberration, Doppler's principle and Fresnel's law for the velocity with which light is "dragged along" by a moving body through which it passes.

In order to account for the negative result of Michelson's aberration-experiment, Lorentz assumes, as was done independently by Fitzgerald, that a body moving through the ether diminishes in dimension in the direction in which it moves.

Particular interest attaches to a further development of the theory in the direction of an explanation of gravitation on electro-magnetic principles. In a paper published in 1900 Lorentz shows, first of all, that gravitation cannot be explained by assuming that bodies are constantly emitting electro-magnetic radiations of very short wave-length and high penetrating power, and that gravitation is due to the action of the ether in this disturbed condition on the electrons contained in bodies. Lorentz therefore proposes a different theory which is, in a way, an adaptation of Mosotti's theory of gravitation. Assuming that all bodies contain an equal number of positive and negative ions, it is clear that an explanation of gravitation by the mutual action of these ions, this action being, of course, transmitted by the ether with the velocity of light, can only succeed if it is assumed that in some way the condition of the field produced by a positive ion differs from that which is due to a negative ion; the action of the fields of the two kinds of ions on other ions is then supposed to be different, but in such a way that the action of a + field on a + or − ion is the same as that of a − field on a − or + ion respectively. Hence it follows that there will be no *electrical* action between two bodies containing ions, *i.e.* no tendency to separate the positive and negative ions, but a resultant action which constitutes what is observed as gravitation.

Lorentz then goes on to show the effect of the motion of attracting bodies in modifying the ordinary law of gravitation, and here he arrives at a remarkable result. The deviations from Newton's law depend on the ratios of the velocities of the bodies to the velocity of light, but only on the second power of these small ratios. Hereby he removes the grave difficulty first pointed out by Laplace against the assumption of a propagation of gravitation with a finite velocity, unless this velocity was millions of times greater than the velocity of light. By the peculiar way in which the condition of the ether is disturbed by a moving ion the effect of the motion on the apparent attraction is of a higher order of smallness, and, in fact, so small that no arguments can be drawn from astronomical data in their present degree of accuracy against the assumption. The latter result is independent of the special form which Lorentz gives to this theory, but holds for any electro-dynamical theory of gravitation on similar lines. Thus it looks as if there were no objection to applying this important unification to our physical theories. How Lorentz's work, some of it well known to every student of physics, is appreciated outside the narrow limits of his own country was shown not so many years ago when he received a call to the University of Munich to be Boltzmann's successor, an offer which he did not accept; and again in the end of last year, when physicists of all countries united in honouring him on his semi-jubilee as a doctor of physics. The *livre jubilaire* presented to him on that occasion contains some sixty contributions, about twenty of which are due to Dutch physicists, several to Lorentz's own pupils.

Not in the work of his pupils only do we trace Lorentz's hand; much of the work done by the Dutch physico-chemical school has been to a certain extent dependent upon his collaboration. In the book just mentioned we find this authoritatively declared by Bakhuis Roozeboom, the creator, we may say, of a new branch of physical chemistry, viz. the application of the phase-doctrine to all kinds of equilibrium. As one of the latest applications of this theory, we mention the attempted, and already partly successful, disentanglement of the iron-steel problem by le Chatelier, Roberts-Austen, von Jüptner, and Roozeboom himself (*Zeitschrift für physik. Chemie*). This application is instructive in showing how purely theoretical investigations may suddenly begin to bear upon highly practical problems and be applied for industrial purposes.

Roozeboom's pioneer work was carried out when still in the laboratory of the Leyden University. He is now at Amsterdam as van 't Hoff's successor. In his laboratory we find, working on independent lines, one of van 't Hoff's pupils, Dr. E. Cohen. Of the many investigations carried out by Dr. Cohen none is of more general interest than that on the enantiotropy of tin, partly carried out in conjunction with Dr. van Eyk. Tin—the white metal as we use it—has been known frequently, under the influence of intense cold, to change its condition completely by turning into a grey modification of lower specific gravity. This fact was known to the ancients, and the literature on the subject which the authors took the trouble to bring together forms quite a bulky collection. Nobody had succeeded so far in clearing up the chaos which surrounded the phenomenon and its explanation; this has now been done in the papers referred to. It appears that the change from white into grey tin is a reversible phenomenon, the transition temperature being 20° C.; this point was determined both with the dilatometer and electrically by the modern method of transition-cells. The transformation of white into grey tin goes on with increasing velocity the lower the temperature down to −50° C., after which it decreases rapidly. The existence of a maximum in the rate of transformation is in accordance with what occurs in the transformation of solids and liquids, *e.g.* the solidification of an under-cooled liquid (Tammann). The velocity is increased (1) by the addition of a little grey tin at the beginning; (2) by the addition of pink-salt; (3) by exposing the tin to the low temperature for a long period, or by alternately cooling and warming it. Above 20° the grey tin is transformed into the white modification with very rapidly increasing speed the higher the temperature. Measurements have been made up to 40°.

From the above experiments it appears that the whole of our tin-world, except on a few exceptionally warm days, is in an unstable condition. Dr. Cohen is now trying to establish the existence of similar transition-points for other metals.

Van Bemmelen's recent work on absorption and the properties of jellies is looked upon both by chemists and by physiologists as fundamental. In his researches on jellies he has struck out a new line in making accurate determinations of the relation between the vapour pressure of the jelly and its composition. One of the several new points discovered in that manner is that jellies, when taken through cyclic transformation, show hysteresis-phenomena, a circumstance which would not occur if the equilibrium between the jelly substance and the water was of a purely thermodynamical character, in which case the phase-rule with its consequences would hold. The equilibrium in the jelly depends upon its history, which is in accordance with the hypothesis that capillary forces are at work. Van Bemmelen looks upon a jelly as a system of two phases—a solid mixture of the colloid and water, and, embedded in the interstices of this mixture, water. In some jellies this solid part shows remarkable sudden transformation into a modification of different composition, but there is no indication of the existence of hydrates. It will interest the reader to hear that Prof. Bemmelen, having recently reached the age of seventy, has become a "professor emeritus" of the Leyden University. In the light of his recent experimental work there is some humour in the Dutch law considering a man of seventy unfit for a professorship. Van Bemmelen is succeeded by Schreinemakers, who may be described as Roozeboom's *alter-ego* (I am speaking from a scientific point of view).

Each country has its own bread, its own type of boots, its own characteristic music—can the same be said with regard to contemporary science? Looking broadly at the nature of the scientific work which is undertaken in different countries, and the manner in which the work is carried out and put before the public, we observe differences which are the natural manifestations of national characteristics. At the same time, these differences are chiefly external, superficial. No science, not even any special branch of a science, is now the property of any one nation. What appears to direct the exertions of the men of science of a country along particular lines more than national character is the influence of the few eminent men which the country is fortunate enough to possess. This influence in a small country like Holland is obvious even to the casual observer.

The origin of the young Dutch school of chemists is no doubt to be traced to van 't Hoff. In the same way we might speak of a Dutch school of which van der Waals is the origin. Those who want to acquaint themselves with the work done recently in this branch of physics are referred to the new edition of van der Waals' book on the continuity of the liquid and gaseous conditions. (German. Leipzig: J. A. Barth, 1900.) It is unnecessary to say anything of the first volume, which is a reprint of the former edition, and a translation of which has been available for several years. The second volume which has been added to the book contains van der Waals' theories of mixtures of two substances in the liquid and gaseous conditions. First of all we find a reprint of van der Waals's paper of 1890 in the *Archives Néerland-*

aires and the *Zeitschrift für physikalische Chemie*, and, secondly, later developments and additions and the application to recent experimental work, most of which was carried out by Prof. Kamerlingh Onnes and his pupils at Leyden. Van der Waals pays an eloquent tribute to Prof. Onnes' merits in this direction in the dedication of this second volume. It appears that but for him the original theory might never have been published and would certainly not have borne any fruit.

The importance of a theory of mixtures, as of other theories, lies in this, that it may show the connection between a number of phenomena which otherwise have to be treated separately, and may, directly or indirectly, bring to light new ones. That a theory was urgently wanted in the phenomena of mixtures even of two substances need not be set forth. In a theory of mixtures we may distinguish different parts, more or less independent of each other, which together form the whole. It consists firstly of an application of thermodynamics to find out the rules for the coexistence of phases—the gas and the one or two liquids. To do this it is only necessary to assume the experimental fact that the properties of mixtures form a continuous series between those of the components and, therefore, that a mixture has an equation of condition of the same general features as that of a single substance. Van der Waals is not the only man of science who has been working on these lines, although doubtlessly the first who conceived the notion of such a theory. Not only had special problems relating to mixtures been successfully treated by Konowaloff and others, but Duhem, applying his method of the thermodynamic potential, had been working in the same direction. In the reviewer's opinion, however, it cannot well be denied that the method used by van der Waals in attacking the problem by means of the "free energy" ψ, and its graphical representation, is by far the most effective and the safest guide amidst the intricacies of the problem.

Leaving alone questions of priority, we may say that the theory as sketched has led, more or less directly, to the complete disentanglement of the critical phenomena of mixtures, to the tracing of the proper features of the various diagrams between the pressure, volume, temperature and composition, and to the discovery of various other new facts, such as the existence of maxima and minima in the critical temperature and their connection with minima and maxima in the vapour pressure, and the influence of pressure on the coexistence of two liquids. All these and many other points are fully set forth by van der Waals in this second volume.

Van der Waals has not, however, contented himself with that; from molecular considerations he was able to deduce an equation of condition for mixtures of a definite form, depending, as does his well-known formula for single substances, on attraction-constants a and volume-constants b. It is somewhat to be regretted that in the original paper no attempt was made to guide the reader in ascertaining in how far special results arrived at were dependent upon this special equation or not; everybody will feel the importance of the distinction, and certain controversies which have arisen in connection with the theory would have been prevented by a clearer distinction on this head.

The importance of this point has increased lately in connection with the modern conception of normal (non-associating) and abnormal (associating) substances. Van der Waals' equation can be used for normal substances as an approximate guide, although even for these the approximation is very rough and hardly amounts to more than a certain resemblance, at least at small and medium volumes. For abnormal liquids the equation cannot even profess to do that, and van der Waals' results, in so far as they depend upon this equation, are not applicable to these substances at all. Lehfeldt has noticed that, so far as we know, normal liquids mix in all proportions and that partial miscibility occurs when at least one of the components is abnormal. Van der Waals' theory does not confirm this, inasmuch as such values may be assigned to the constants in his equation as will lead to partial miscibility. At the same time, as no normal liquids of partial miscibility have been discovered so far, this subject is outside the scope of van der Waals' equation.

The reader must not get the impression that results deduced for normal liquids by means of van der Waals' equation are of small value owing to the inaccuracy of the equation. An instance will illustrate this. Van der Waals discusses the question, also treated by Ostwald and others, what function of the composition of a mixture its vapour-pressure is. He arrives at certain conclusions, one of which is that there cannot be more than one maximum or minimum, at least that the combination of a maximum and a minimum is very unlikely. Guided by this result, Hartman (Leyden) has discovered that there is an obvious error in Konowaloff's result for propionic acid and water, the curve for this combination being in contradiction to Konowaloff's own measurements, and Kohnstamm, working in van der Waals' laboratory, similarly discovered an error in Linebarger's result for benzene and carbon tetrachloride, a result which, if it had been confirmed, would have been even more striking, as both these substances are normal. On the other hand, Caubet and Duhem maintain to have realised the double phenomenon in question with methyl chloride and sulphur dioxide; if the latter result were confirmed it would certainly show in a striking way with what extreme caution conclusions drawn from the approximate theory have to be accepted.

Owing to the recent establishment of a "van der Waals fund," the famous author is now in a position to conduct experimental researches in his own laboratory. Several valuable memoirs have already appeared under this trust.

A very interesting departure has been lately made by Kamerlingh Onnes and his pupils to construct plaster models of the ψ surface entirely based on experimental data. Models of that kind will no doubt become a powerful assistance in the understanding of the intricate phenomena displayed by mixtures.

Turning our attention towards the work which is being done in the Leyden laboratory, we notice researches which are being carried on relating to Hall's phenomenon, the magnetic rotation of the plane of polarisation and many others. A special feature of the work is the constant use of low temperatures down to the boiling point of air. We feel at a loss what particular part of the work to review specially; in the small space available no justice could be done to any one without being unjust to others, and we abstain from reviewing anything in particular, considering that the "Communications from the Physical Laboratory at Leiden" are widely distributed and will, no doubt, be sent to anybody interested who takes the trouble to apply for them.

Much else might have been mentioned in this review, but we have tried to select that which would find the largest number of interested readers. J. P. K.

MAXIME CORNU.

THE hand of death has been heavy on the French botanical world. In recent years it has fallen successively on Duchartre, Baillon, Naudin, de Vilmorin and Franchet: all men in the foremost rank, whom their fellow-workers in England counted as sympathetic friends. And now the untimely and unexpected death of Maxime Cornu has come upon many of us—and not least at Kew—as a personal grief. I saw him last autumn in Paris full of the business of congresses into which he was throwing himself with irrepressible vivacity